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MENTAL LIFE OF TWO *MACACUS RHESUS* MONKEYS IN CAPTIVITY.—II.

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NUMBER TESTS.

There has been considerable written and but very little done toward a rigorous examination of the number notions of lower animals. Stories of their wonderful achievements in counting and comprehending numerical relations are abundant. For example, it is said that shepherd dogs count sheep. One drove sheep to the wash in groups of ten each. Bird dogs are said to count the number of birds that fall when the master fires. One dog counts the railway stations when on a train, and so knows where to get off. Another displayed "thorough proficiency in the first four rules of arithmetic," barking off the answers of the problems put to him. A mouse came nine times to carry away each time one of her young handed to her from a cup, and did not return after the last was taken.[1] A Cincinnati mule counted fifty.[2] A dog counted her six puppies and knew when one was missing. Leroy reports a crow that counted four. One of the nearest approaches to real counting appears with some insects. A species of wasps, the Eumenes, supplies for its prospective young five victims for each egg laid. Other species with constancy supply ten, fifteen and twenty-four. When the regulation number has been put in the wasp stops even though some of the victims may have been stolen in the meantime. Again it is said that the Eumenes supplies for each male egg five and for each female egg ten victims. This looks

[1] Lindsay, Vol. I, p. 451.
[2] Weir: Dawn of Reason, p. 173.

very much like counting on the part of these insects. On the part of monkeys it is said that the larger apes will approach two or three men, but will not attack a larger number. Monkeys have been taught to hand up one, two and three marbles or straws when these numbers were called for.[1] Possibly all of the above cases were mere associations with quantity, and that the number idea as such was wholly absent.

I am not aware of any very systematic attempt to test the number sense in the lower animals. Mr. Garner made a few tests with monkeys, using marbles, from which he concluded that the monkeys knew clearly the difference between two and three. He presented two plates, on one of which were placed three cubes of carrot or other food; and on the other, one. The monkey tried to get the food from the plate containing the greater number. It is probable that quantity was more the basis of choice than number. Yet when one piece was increased in size the monkey still tried to get the two. Next he put three marbles into a box having a hole in one side of it. After the monkey had taken them out one at a time, for several times, he then put only two into the box. The monkey felt in the box, and then looked around where he had been sitting as if to find the missing marble. While Mr. Garner was well satisfied from these tests that the animal could distinguish number, these cases are easily explained on the basis of association and suggestion.

I was unable to make tests similar to Mr. Garner's with the *rhesus* monkeys because they were so wild and did not engage in play. I have tried to approach the question, however, in another way. I devised a special apparatus with which I made 2,790 tests with the male, 1,260 with the female, and 140 with two children. These, like the experiments of others, can hardly be said to test the ability to count or to comprehend number, but the reactions were so unique that it seems worth the while to report them rather fully.

The apparatus used consisted of a board 2½ inches wide and 10 feet long. Twenty-one uniform wide-mouthed bottles (or for the female, glasses) were set on the board four inches apart. The bodies of these bottles were covered with white paper to prevent food from being seen except from immediately above. In experimenting I set this board down at right angles to a line from the monkey to the middle of the board, and then stepped back from three to six feet. After a few tests had been made it became apparent that it would be most convenient to make one presentation in the south and one in the north end of the room. The position of the apparatus varied

[1] James Weir: Dawn of Reason, p. 177.

from time to time in each end of the room about ninety degrees in the angle of placing, and from a few inches to four or five feet forward and backward and from right to left.

The male was experimented with first. Food was put into the fourth bottle from each end of the board. By mere chance he first fell upon the food in the bottle on my right. In all the succeeding experiments only the right end of the board was used. The male was tested with eleven food bottles in the following order, counting from the end of the board to my right, 4, 2, 5, 1, 6, 9, 11, 8, 3, 10, and 7. In the accompanying table all above six are omitted, so the table gives the order 4, 2, 5, 1, 6, 3, which was the order followed with the female. The female was tested with no numbers higher than six. The numbers in the first column at the left indicate the numbers of the bottles or glasses used, counting from the end of the board to my right as I stood behind it. The numbers in the vertical columns following this indicate the number of times that each of these vessels was approached directly by the monkeys in a series of thirty tests. The number of times that vessels above the 7th were approached directly are thrown together in the table as "over 7." These tables take no account, again, of the bottles or glasses looked into after the one approached directly. The results from feeding the male in 7, 8, 9, 10 and 11 are not shown in the table.

The accuracy of selection could not be due to accidental markings on the vessels. For after accuracy was established I exchanged the food vessels for vessels taken from the other end of the board. These exchanges caused no apparent disturbance in the accuracy of choosing. It is interesting to note how gradually the burden of his choice swung up and down as the food was changed from one bottle to another. For example, I will give rather fully the changes when the male was fed from bottle 6. Note that he had been fed previously in 4, 2, 5, and 1. (1) In the first and second tests he went directly to 1, then examined all of the bottles up to 6. (2) With the third test he began going to 2 and 3, and then looking into the bottles back down to 1 and then up to 6. (3) With the 10th test he began going to 3 directly then to 2 and back to 6, thus neglecting the return to 1. (4) With the 16th trial he began going directly to 4, tracing down to 2 and back again to 6. (5) With the 27th trial he began going directly to 4, then looking into 3 and returning towards 6. (6) With the 42nd test he began with 4, and then took 5 and 6, leaving off all of the lower numbers. (7) With the 44th test, 5 was approached directly for the first time. (8) With the 53rd test he went directly to 6 for the first time. (9) With the 91st test he began choosing some numbers higher than 6.

There were, of course, numerous relapses after each one of the beginnings noted above. With the 90th test experimenting closed for the night. The next 30 tests show scattering choices as a result of the intermission. Bottle 1 was never chosen directly after the 9th test; 2, but once after the 13th test; and 3, but once after the 133rd test. The effects of these over-night intermissions are apparent in the table for the male. With food in 2 it appears in the seventh thirty; with the 5, in the fifth 30; with the 6, in the fourth 30; and with the 3, in the seventh 30. The over-night intermissions were avoided with the female.

One, two, five and six were definitely located, while three and four were always over-estimated. On the whole four was greatly over-estimated by the male. Only 22 times out of the 270 trials did he go directly to numbers below four, while he went directly to numbers above four 187 times, and directly to four 61 times. Three had 22 choices below it, 129 at and 149 above it. The apparatus was new for 4, while 3 was approached with considerable experience. Yet three had been preceded by feeding in 9, which would tend to induce over-estimating it. I was unable to note any irregularities in the condition of the animal or method of work that would account for the comparative failures with 3 and 4 followed by rather successful associations with 5 and 6. The choosing of 5 so often when I was feeding him from bottle 4 almost led one to believe that if the experimenting had begun with 5 instead of 4 he would have chosen it more frequently than he chose 4. Yet this is only a surmise. With 5 he selected lower numbers more frequently than higher ones. With 6 the former feeding bottle favored the selection of lower numbers. The table and the above analysis both show how he was affected by this fact. In all the higher numbers the choices fall mostly below the number of the food bottle. Thus, 7 was chosen 72 times with 8 choices over 7, and 220 choices under 7. Eight was chosen 127 times with 50 choices over 8 and 93 choices under 8. The choosing level was here lifted up by the fact that 11 had immediately preceded it. But the number of direct choices of 8 decreased, as did also the over-choices, as the experimenting progressed. Nine was chosen 25 times with 4 choices over 9 and 271 choices under 9. It had been preceded by 6. Ten, preceded by 3, had 1 direct choice, 2 over choices and 297 under choices. The center of choices for 10 fell with number 7, as it did also for 9. Eleven was preceded by 9. Its predecessor, therefore, lay closer to it than did that of 10. Eleven had 15 direct choices, 1 over choice and 194 under choices, while the main body of choices lies with 10. Four and five then seem to be a turning point in his reaction to this apparatus. With 4

MALE

Numbers of Bottles	Food in Four									Food in Two										Food in Five						Food in One				Food in Six								Food in Three									
1	1												1															17	25	3												1		1		1	
2										1	3	5	9	9	3	1	1	23	25	4	1						5	12	5	5			1						1		2	6	3	1	2	1	3
3		1	4	2			7	5	2	10	14	14	12	16	22	7	19	7	5	8	9	6		2		12	22	1		17	7	5	4	3	1			1	8	9	13	15	14	13	20	17	19
4		5	5	6	6	9	8	12	10	10	8	10	5	5	5	19	9			8	16	15	7	13	5	13	2			5	17	7	16	6	2	2		4	11	11	12	8	13	12	6	9	8
5		10	10	7	15	15	8	9	12	4	5	1	3			1	1			7	1	8	18	13	20	5	1				5	15	7	10	8	12	3	7	7	9	3			2	1	2	
6	1	1	9	11	8	5	7	4	4	3						1				2	2	1	5	1	5						1	3	0	9	15	12	26	13	3	1				1	1		
7	3	6	1	4		1			1							1				1	1			1									1	2	4	3		2									
Over 7	25	7	1		1				1	2																							1			1	1	3									

FEMALE

Numbers of Glasses	Food in Four						Food in Two						Food in Five										Food in One			Food in Six										Food in Three						
1	6	3	1	1	1		3	8	5	1	4		2					1					1	11	25	2										1	1					
2	4	4	1	3	4	12	14	14	21	21	19	26	9	1		1	2	3	2	3	2			10	5	1										3	1	3	5	5	8	4
3	3	9	11	5	8	12	9	6	4	8	7	4	15	21	11	6	13	6	6	8	8	3	16	8		7						2			3	2	7	17	18	12	19	23
4	5	4	11	14	13	5	3	1					2	8	11	13	11	10	17	10	9	11	9	1		4		1			1	1	3	1	2	9	14	7	7	13	3	3
5	7	8	2	1	3	1	1						2		6	8	3	7	3	8	9	12	3			5	5	3	1	1	4	3	3	5	5	13	5	3				
6	2		4	3	1			1							2	2	1	2	2	1	2	4	1			2	6	6	7	8	7	10	10	12	14	1	1					
7		1		1																						7	9	4	8	12	8	9	7	9	4	1						
Over 7	3	1		1														1								2	10	16	14	9	10	5	5	3	2		1					

and lower numbers he inclined to choose higher numbers than the food bottle; above 4, lower numbers were chosen. While numbers were learned up to six, 3 and 4 were beset with special difficulties. Three was more definitely recognized than 4. One and 2 gave comparatively little difficulty, and in the case of 4 it may be that the confusion arose out of the fact that it was near this turning point of approach.

With the female the tables of results show less capacity than with the male, but a more ready association for the numbers that she was able to comprehend. Thus, when the food was in 1, the male chose correctly only 17 times in the third group of 30 tests, while the female chose correctly 25 times. He chose two three times in the sixth thirty; she chose it 26 times; he chose three 13 times in the seventh 30; she 23 times. So far she chose better than he did. Above 3 his superiority for the number idea, if indeed it be number, is very apparent. He chose four 9 times in the sixth thirty; she chose it only 5 times. He chose five 20 times in the the sixth 30; she only 7 times. He chose six 26 times in the eighth 30; she 11 times. It was thought unnecessary to try her further. Five was as difficult for her as seven for him. She learned no number above three with any degree of certainty. The male learned six as perfectly as the female learned three. His central number space from which he appeared to work down to a lower and up to higher numbers was between four and five, hers was between three and four. But she introduced a variation with six. Here she cast most of her choices above the food glass. Thus with her 1, 2 and 3 are apparently numbered, or at least recognized definitely. Four and five are quantities just to be estimated. With the male 1, 2, 3 (4?), 5 and 6 are definitely recognized numbers, while all beyond these are an indefinite great many. We may conclude that these two animals with this apparatus were able to recognize numbers from 1 to 3 and from 1 to 6 respectively.

Two children, aged 3 and 5, who had not been taught to count, were brought before this apparatus under conditions as nearly as possible the same as those for the monkey. Marbles were used instead of food. The older child located 1, 2 and 3 perfectly, but could not make sure of higher numbers. It is possible that he could have succeeded with others if experiments could have been continued without fatigue. The monkeys apparently never fatigued no matter how long the experimenting was continued. This child early resorted to noting the spatial location of the glass containing the marble, and attempted to return to that place in the room. He was not allowed to take the marble unless he came directly to the glass containing it, but in case he missed it he was allowed to look

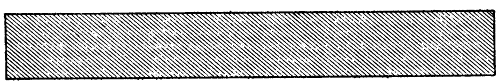
along the row of glasses and to see in what glass the marble was to be found. As he returned to his mother at the other side of the room the apparatus was shifted to a new position. When he began trying to locate the glass in this way he was told that the apparatus would be moved, and arrangements were made for sliding it over on to a second table. The three year old child learned 1 and 2 perfectly. Three was more difficult, and the child failed with larger numbers. Thus the monkeys appear in this particular to attain in one year to a development attained by human beings in from three to five years. But here monkeys ability to comprehend numbers and number relations probably reaches its limit, while the human being goes on not only to a comprehension of larger numbers but learns to deal with numbers both abstractly and symbolically.

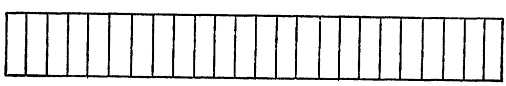
But what is it that the monkey and children recognize here? Is it number, quantity or form? Lubbock would have it that in all such cases as mentioned at the beginning of this section, which closely resembles those with the monkeys and children the animal does not have a number idea, but an impression of greater or less quantity. It could hardly be maintained that these monkeys and children counted the glasses or bottles in order to determine which one contained food. With the larger numbers clearly selected one could see the eyes of the monkey give a quick movement along the line. The movements were not those of stops and starts. Roughly estimating I should say that the glance did not occupy more than one-fourth of a second. The appearance was merely that of sizing up of the quantity. Ribot[1] seems to express the process exactly when he is discussing Leroy's report of the crow. He says: "I see here not a numeration but a perception of plurality, which is something quite different." Of this process he adds, "it is a preliminary state, an introduction, nothing more, and the animal does not pass beyond this stage, does not count in the exact sense of the word."

Lloyd Morgan[2] has dealt definitely with the case in hand. He says: "The raw materials of numerical relations, as of those of space and time, are given in our daily experience, and are marginally sensed long before they are focally perceived. The child, long before he can count, senses the difference between one thing and two things, between two and three, between three and several, between several and many. It would not be surprising to find that a clever dog was able to distinguish from each other playing-cards, from the ace to the ten. But they would be distinguished through difference of sense-impression,

[1] Evolution of General Ideas, Chicago, 1899, p. 21.
[2] Introduction to Comparative Psychology, London, 1900, p. 232.

not through perception of numerical relations. So, too, with succession. One can very readily distinguish a succession of three from a succession of four, without anything like counting, through the sensing of sense-experience. It is, indeed, surprising how large a group of sounds, up to sixty-four in my own case, can be appreciated without counting. But the perception of numerical relations is something more than the sensing of a group of discrete objects or sounds. It is also to be distinguished from the perception of the group as larger or smaller. Whether the numerical relations were first perceived among objects simultaneously presented, or in association with succession, we cannot say; but it is at least possible, if not probable, that they arose in close association with that phase of time-experience which presents us with succession rather than duration. Run the eye slowly from left to right along the shaded diagram. You are subconsciously aware of the duration of the impression it produces. But if you run the eye along the second figure you are aware of succession.



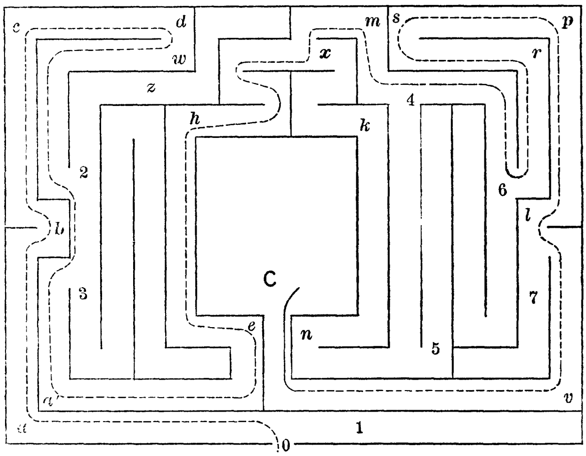


The homogenous duration of a continuous impression gives place to a successive series of similar impressions. And in this series you have not only one aspect of time-sequence, but also the material form from which a numerical-sequence may, on the advent of reflection, be evolved." This appears to be what the monkeys and children did in their reactions to this apparatus. The numbers 1, 2 and 3 were clearly discriminated, while 4, 5 and 6 were seen as a somewhat definite mass. Beyond these we have only an indefinite mass or group but no measured quantity.

Reaction to a Maze.

The maze used was identical in form with that used by Dr. Willard Small[1] and I am indebted to him for the use of the accompanying cut. My maze was, however, of necessity much larger than his, being 17 feet long, 13 feet wide and 14 inches high. The alleys were 1 foot wide. The whole was built of "chicken wire" fastened to wooden frames. The central por-

[1] *American Journal of Psychology*, Vol. XII, Jan., 1901, p. 207.



tion was not covered, so that when the animal procured his food, which was put in the middle of this central portion, he was free from the maze and could return to his cage. The entrance was at *o*. Numbers 1-7 indicate the blind alleys. The dotted line shows the most direct course through the maze.

The sources of error, on the part of the animals, were the blind alleys and return movements along the direct course. In keeping the records, entrance into a blind alley was always called an error. Also, if the animal proceeded to any point in the direct course, then returned any distance in this course, turned about and proceeded correctly thereafter, he was recorded as having made one mistake no matter how great the return movement. If, for example, he proceeded to 3 and returned to *a*, this was counted as one error. If he entered 3 and returned to *a* this counted for two errors, one for entrance into the blind alley and one for failure to take up the proper course as he came out of it. Entrance at 4 was arbitrarily counted as an error, while a trip through *xkn* was not so regarded as that merely represented a longer course to the goal than proceeding by way of *m*. The shortest path from the entrance to the food was 105 feet, and there were 27 corners to be turned.

Method.

After the cage had been brought up to the entrance of the maze, the monkey was allowed to see some food placed on the floor in the middle of the open center. The door of the cage was then lifted. The monkey invariably rushed at once into the maze. Printed forms, identical with the accompanying cut, were used for keeping the record of movements, while the time

was kept with a stop-watch. I took up my position behind the cage and opposite *o*. This brought the maze in full view, and allowed the monkey to see me from any part of it. While he moved I traced his path on the forms indicating stops and the number of seconds required to reach any given portion of the maze along with the time spent in resting.

As soon as the animal had procured his food the cage was turned partly around and the door opened. After a few times he learned to go immediately across the maze into the cage, even while I held the door open. On beginning it was decided to regard the maze as learned whenever the animal succeded in passing through it ten times consecutively without error. The male reached this standard with the 113th trip, and the female with the 66th.

Analysis of Results.

In their native habitat these monkeys are accustomed to weaving their way through the boughs of trees and along winding paths of the bamboo thickets.[1] Their native haunts in a very general way at least are like the maze. They showed no fright at being confined in it and my presence near the maze (I was about ten feet from it) did not seem to annoy them in the least.

In the first tests if a blind alley was entered it was pursued to the end. Then the monkey seized the wire and shook it, looked about awhile and returned. Later when alleys were entered there was no shaking. He either sat as if trying to determine his proper course or turned about quickly and came away. After an alley had been entered several times it was pursued only a short distance. They were entered and left at about the same speed. The hasty glance, when blocked in an alley, followed by the return movement, gave the animal very much the appearance of saying "Hold up! No, that 's wrong. Well, I 'll go back."

In the accompanying table the figures in the first horizontal line indicate the number of the trips through the maze. Twenty trips for each animal are shown. The items in the column at the left will be readily understood by reference to the preceding cut.

A successful passing of a blind alley (4 has been counted here as a blind alley) is indicated by leaving the space blank.

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[1] Kipling says in his Jungle Book, p. 57, that the flight of the Monkey People through tree-land is one of the things nobody can describe. They have their regular roads and cross-roads, up hills and down hills all laid out from fifty to seventy or one hundred feet above ground, and by these they can travel even at night if necessary. This is quite a correct description of their habits.

MALE

Parts of the Maze, etc.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Entered Blind Alley 1	4		5	1	5	1	2	3	1	1					1			1	1	
a, Passed	12		12	2	8	2	4	4	4	2		2			2			2	2	
b, Passed	12		8	2	10	2	6	4	4	2		2			2			2	2	
c, Passed	10	2	6	2	8	2	6	4	4	2		2			2			2	2	
w, Passed	16	4			6	4	6	4				2			2			2		
Entered Blind Alley 2	8							2												
Entered Blind Alley 3	11	2	1			1	1											1		
e, Passed	2	4			6	2	6	2		2										
e h, Traversed	30	12		1	16	4	8	2		2	2									
x, Entered		3		2	5	1	3	1												
kn, Traversed	2	16	3	5	23	3	7	2									2			
Entered Blind Alley 4		2	1	2	3			1	1	1	1		1	1		1	1			
" " " 5		3		1	3		1							1			1			
" " " 6	6		1	1			1		1	1	1	2	3	2	1	1	2	1	1	1
r, Passed	6						2				2	2	6	2						
p, Passed	2				2		2									2				
Entered Blind Alley 7	2						1	2				1				1				
To v and Return	1				1															
Times in Seconds	2700	1800	420	180	900	230	390	236	160	204	107	100	83	61	67	43	69	30	66	41
Errors	56	32	15	8	32	6	15	11	4	6	3	4	4	4	2	3	4	4	2	1

FEMALE

Parts of the Maze, etc.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Entered Blind Alley 1	4	3					1	1	1	1										
a, Passed	8	18					2		2	1	2									
b, Passed	8	24					2	2	2		2									
c, Passed	8	22		2			2	2			2									
w, Passed	2	14		2																
Entered Blind Alley 2	1	5		2			1	1						1		1	1	1	1	
Entered Blind Alley 3	2	8		1		2	1	1				1		1						
e, Passed		24	2	2	2	2								2						
e h, Traversed	2	30	2	2	2	4														
x, Entered	1	13																		
kn, Traversed	7	30			1															
Entered Blind Alley 4	1	11	1	2	2	1	1													
" " " 5	1	5	2		1	1														
" " " 6	1	4	1	1	1	2	1	1	2	1	1	1								
r, Passed		4	2	2					2											
p, Passed	2	2	2																	
Entered Blind Alley 7	2	1	1																	
To v and Return																				
Times in Seconds	3300	7920	840	420	375	450	210	195	165	135	250	154	76	164	45	175	94	116	158	54
Errors	23	76	9	11	5	7	5	5	3	3	2	2	0	4	0	1	1	2	1	0

The figures indicate the number of times that any point was passed or any alley was entered by way of error. Some short return movements between the letters on the cut could not be presented in this table. Also many return movements along

the direct course passed several of the points designated. Hence, often the total number of errors given near the bottom of the table does not agree with the items of error in the body of the table. It will be seen that the errors decreased very rapidly after the second trial, and that the female was the slower at first, but surpassed the male in her fifth trip. For her first trips she required the longer time, but he made more than twice as many errors as she did. His movements were much more rapid than hers, and she spent by far the greater amount of time sitting around with folded hands.

Alleys 1 and 6 persisted longest, while 2, 5 and especially 7 were soon eliminated. Yet relapses sometimes set in where a part had been passed successfully a great many times. The table shows that the male learned the last part of the maze first, but suffered considerable relapses with the 11th, 12th, 13th and 14th trips. The middle part was well learned in the tenth trip, while in the first part of the maze serious relapses occurred after the 12th. With the female we find the same order, but the parts were mastered more readily. In her 13th trip she went through without error, but the male did not meet with similar success until his 36th trip.

Comparing the two in the accompanying table of average times and errors it appears that the female has accomplished much more than the male. She learned the maze in 66 trials, while he required 113. His movements being much faster than hers he attained to an average time of 44.8 seconds, while her average never went lower than 55. But when in the maze neither ever went faster than a brisk walk or "dog trot," though, as with Mr. Small's rats, this gait was hastened toward the last of the journey. The increase in speed, however, was not very great, and was rarely begun until after *p* was passed. When the latter part of the maze was fairly learned, the monkeys often after passing *r* would begin to smack their lips audibly, apparently at the thought of the food. In their earlier trials as they passed from *e* to *h* they stopped to look at the food, and often attempted to reach it, but after the first few trials no effort was made at shaking the wire or reaching, and often they did not even look toward it.

The feeling of uncertainty manifested itself with the male at *m* and 4. Coming from *n* up through 4 he would not turn toward the exit, but would proceed to *m* and then face about and continue correctly. The male repeated this movement ten times. The female showed signs of similar indecision by half body-length movements into 3, 4 and 6, and three times by turning completely around when passing 4. Often there was a mere hesitancy at 4 before continuing the trip. On entering the maze the first time the animal's first move was to get nearer

MAZE RESULTS.

		1st-10	2nd-10	3rd-10	4th-10	5th-10	6th-10	7th-10	8th-10	9th-10	10th-10	11th-10
Average Times in Seconds.	MALE.	726.9	73.7	76.	64.4	61.2	67.2	53.8	46.6	44.8	45.4	59.5
	FEMALE.	2579.	127.7	63.6	68.1	55.8	55.					
Average Errors.	MALE.	18.5	3.1	3.2	2.2	1.4	2.6	1.5	1.6	1.2	.7	.4
	FEMALE.	14.1	1.3	1.7	.7	.4	.3					

The consecutive trials have been thrown into groups of ten each. The numbers in the vertical columns indicate, respectively, the average times and errors for the corresponding groups. The last three trials for the male and the last six for the female are not accounted for in the above tables, being excesses above full tens.

the food which he saw in the center. This, along with his trials and accidents, accounts for his getting as far as to *n*. After

this mere accident, apparently no curiosity, along with what seemed to be a feeling of disgust at having failed to procure the food induced him to make his way through the remainder of the maze. In the earlier trips the sight of the food was the leading stimulus. But it played a smaller role in proportion as the maze was learned. Once when free in the room the monkey entered the maze and made the trip as if in the hope of food as a result of the process though no food was in sight at any time. Evidently the food is imagined as a final outcome of the trip. The smacking of the lips when some distance from the food and headed in the opposite direction would seem to warrant this conclusion.

A cat was taken through the maze 6 times. *Taken through* is literally correct. The cat cared as much for me as for the food, so after some time I sat down in the center of the maze. But the cat after two hours, having slept 20 minutes in the meantime, had only gotten in next to the center, and appeared to be absolutely incapable of further progress. I then moved over to *p*. In a few minutes the cat was going back and forth near me, but would make no further progress. I moved on to *v*. It followed, paced back and forth and lay down to sleep again. When I moved on to *o* it followed, and presently made the last three feet unaided. In subsequent trials it learned to go the last 12 feet alone, but had to be coaxed as before over the journey up to that point. The cat was less at home in the maze than the monkeys were. It tried to claw the wires apart and to squeeze over the tops of the frames to which the wires were fastened. In its movements it was slow and stupid as compared with the monkeys. It apparently profited less by its experience at the last part of the maze. But its liking for persons was so much greater than theirs that it was somewhat put to disadvantage by my presence.

No other animals were put through the maze for similar comparative studies. It may be of interest, however, to compare the time for the monkeys and one of Dr. Small's rats, although the sizes of the two mazes differ so greatly. The upper line gives the time for the rat for ten trips. The other two lines give the times for the monkeys. All are given in seconds.

780	180	240	105	60	90	180	30	60	120
2700	1800	420	180	900	230	390	285	160	204
3300	7920	840	420	375	450	210	195	165	135

In order to compare the rates of improvement the above numbers have been converted into per cents, regarding the first numbers consecutively as 100 per cent. and computing the per cents for the succeeding times. Arranged in the same order as above they appear as follows:

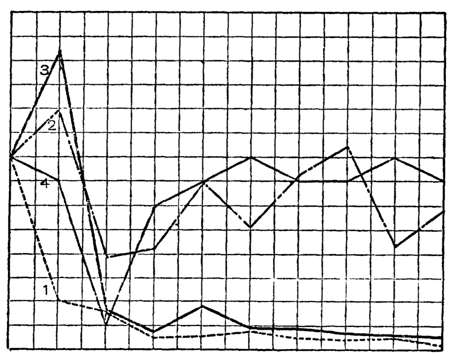
100	23	30.8	13.5	7.7	11.5	23	3.8	7.7	15.4
100	66.7	15.5	6.7	33.3	8.5	14.4	10.5	5.9	7.5
100	240	25.5	12.7	11.4	13.6	6.3	5.9	5	4.1

While the times for the monkeys are much greater than for the rat, the per cents show the rate of improvement for both to be much the same. The longer time may be due to the fact that the maze was much larger. The variations with the monkeys are much greater than with the rat, and the initial improvement is greater with the rat possibly because Dr. Small allowed it to go at will through the maze during one whole night following each experiment. This experience in the maze apparently should have tended to equalize the rat's subsequent trials, since the greatest variations in time and errors occur while the animal is becoming acquainted with the maze.

Comparison of Results.

Inasmuch as I have now presented all of my experimental studies except the memory tests it will be of interest to make a comparison of the rates of mastering the several kinds of apparatus. Accordingly I have made out two sets of curves, one based on the first ten trials, and the other on the tests necessary for establishing a fairly complete association.

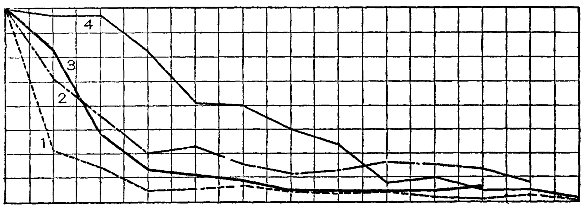
Curves based on the first ten trials. Curve (1) represents



the progress of the first ten trials with the seventeen fastenings and other simple apparatus shown in a preceding section. The time for working the apparatus the first time was taken as 100 per cent., and the nine succeeding times were converted into per cents of this first time. The order of the consecutive trials is indicated by the alternate dividing points along the abscissa. Curve (2) is based on similar computations with the times for the first ten trials with the combination locks. Similarly, Curve (3) is based on the first ten

trips through the maze. Curve (4) is based on the errors committed on the first ten presentations in the tests in colors, forms, and numbers. Twenty-two groups of ten tests were used. The simple fastenings and the maze, curves (1) and (2), after the second trip, are very similar. Curves (2) and (4), (combination locks and colors, etc.), for the first ten tests are very irregular, but strangely enough happen to run along somewhat together.

Curves of Learning. These curves represent the complete learning process. Curve (1), as in the previous set of curves,



is based on seventeen locks, etc., these locks were practically learned in 13 trials. Curve (2) is based on the times of the monkeys with combination lock 1, which was learned by them, and on the first 120 tests with the male with combination lock 2. This was included in the calculation because he had practically learned the combination at that point. These tests were divided into consecutive groups of ten each and the per cents computed as before. In this and the two succeeding curves the consecutive groups are indicated by the alternate dividing lines along the abscissa. Curve (3) is based similarly on the time required for learning the maze. Curve (4) was obtained by dividing the tests for 12 of the forms, sizes and numbers into consecutive groups of ten each counting errors and then computing the per cents as before. In this were used only those series where the association was fairly well established within the 130 tests. Each point then in curve (4) is based on 120 tests and must be regarded as fairly reliable. The similarity of the first, second and third curves is very striking. The elevation of (4), especially at its second and third points, over that of the other curves is probably due to the fact that in a large per cent. of these tests there was first an old and conflicting association to be broken up before the new one could be begun. Progress in the new association would not set in until after 30 tests had been made. Beyond that point the curve is not very unlike the others, however; it shows the same gradual improvement in the associations. While the first few tests with different kinds of apparatus show very different results, as may be seen in the first set of curves, the curves of learning by trial

are rather uniform. Clearly then the first ten or twelve tests cannot be regarded as showing the curve of learning since the results with these vary considerable, not only with the animals but also with the apparatus used. A far better notion of the progress of learning is to be gotten from curves representing results from the first tests to comparative mastery.

The maze, it seems to me, offers no new problem above that of working a combination lock or associating food with one of a series of glasses by number, form or color. The number of trials required for attaining comparative mastery is much the same for each. In the learning process we have here again a more or less definitely directed effort spurred by the food stimulus, fortunate accidents, memory of them and the elimination of useless efforts. Thus blind alleys are cut off, return movements are omitted, the shaking of the maze is dropped, as the path and method of getting the food gradually become fixed upon the monkey's mind. The maze tests, therefore, throw no new light on the general problem of the monkeys' intelligence or method of learning. They confirm the results of the preceding tests, however, and in a general way make an interesting point of connection with Dr. Small's admirable work on rats.

MEMORY.

Memory tests were made three times with the male and twice with the female. The tests were made at regular intervals of fifty days each. The combination locks and maze were used. Each monkey was given ten trials with each apparatus. Probably a comparison of the times for manipulating the locks and going through the maze with the original learning times will give the best notion of the results. The errors run almost parallel with the times, and so, need not be presented.

The first series of memory tests, show reactions shorter than the first ten trials of the original learning, but longer than the last ten trials. The average time required for manipulating each piece of apparatus for the first memory series was about equal to that of the ten trials at the end of the first third of the original learning series. But when the male was taken through these tests after a second space of fifty days he showed a decidedly greater loss than he had shown at the end of the first period, as is to be seen in the table. The female in her second series of memory tests showed a decided improvement, not only in the time required but in her manner of attack, especially in the maze. There she acted precisely as if she knew quite well just how to go through. She made but few errors, and none at all in the last half of the maze. The male, however, by his hesitations, return movements and entrance into the blind al-

	Apparatus.	Average Time for Learning. (In seconds.) First Ten Tests	Last Ten Tests	Tests with time nearly equal to memory time	Average Time for Memory Tests. (In Seconds.) First Series	Second Series	Third Series
MALE.	1st Combination Lock.	34.0	6.4	4th ten 14.4	13.7	22.7	15.6
	2nd Combination Lock.	105.3	13.6	6th ten 27.0	28.1	44.3	66.2
	Maze.	726.9	59.5	2nd ten 73.7	88.3	110.6	157.1
FEMALE.	1st Combination Lock.	89.8	5.1	5th ten 16.2	14.2	10.8	
	2nd Combination Lock.	101.1	19.6	5th ten 32.5	31.0	17.9	
	Maze.	2579.0	55.0	2nd ten 127.7	120.0	44.5	

leys, showed unmistakable signs of confusion, which in his third memory series was still more apparent. In this series he moved less rapidly than in his previous tests, while the female showed herself unusually enthusiastic and increased her originally slow gait considerably, so that while she had formerly required 45 seconds as her minimum time for passing through the maze, she now did it in two separate trials in 31 seconds each. He formerly went through in 39 seconds, but now required 84.

In his third and her second memory series they had been without food the same length of time, and were practically equally hungry. Yet one showed some loss and the other considerable gains over the preceding memory tests and even over the best results in the original learning. It is impossible to say that the monkeys have been equally hungry in all of the tests reported in this paper. Probably they have not been, though every care was taken to have them so. Again, practically nothing is known of just how progressive hunger affects the mental capabilities of animals. All absolute time and error results may have been partly determined, however, by this varying factor of hunger.

In none of these cases is the memory perfect; that is to say, the animal's execution of these tasks was not as good as on the last trials in the original series. Usually the first memory trial

took a comparatively long time and resulted in a large number of errors, but the one trial apparently revived the former images so well that the second trial compares favorably with trials after considerable practice in the original series. This memory, probably, is of the type commonly designated as *associative memory* and does not differ in its essential character from the associative act involved when the manipulation of a piece of apparatus was first being learned. In all such cases the sight of A lead to the expectation of B. If B was imaged, it was done immediately after the presentation of A to the senses. It is possible that B was never recalled between tests however far they may have been separated; it did not recur until the immediate preceding presentation was at hand. In the darkness of his cell the monkey *may* recall in trains of images how objects look, recall something that he did on the preceding day, or may image the special dainty dish on which he dined yesterday or a week ago, but if he does, the fact of such a process can never be known until he learns to introspect and can be taught to write or speak, and that can never be done.

Memory tests with simple apparatus can have but very little value if experimenting has gone on in similar lines between the times of the original and memory tests. It was shown in our tests with simple locks that the horizontal hook which had not been learned at first was worked very quickly several days later, simply because there had been some general improvement in the meantime in dealing with that modality of apparatus. With such apparatus, if experimenting has intervened, an animal might appear to remember very accurately over an interval of fifty days, when in reality his success was due to general improvement rather than to excellence of memory. When there has been no intervening experimenting of any kind it appears that we ought to get better data in case of the animal than we could get with a human subject. It is probable that the animal never recalls the experiment or the apparatus from the time of the original experiment until the memory test is made. With the human being it is quite impossible to avoid the intermediary recalls so that in such cases we do not get a test of the pure permanence of an association based upon a given number of trials in so perfect a form as we get it with the animal. Unfortunately we have as yet no memory tests with animals similar to these reported here, so far as I know, with which to compare them in determining the relative excellence of the monkey's memory.[1]

[1]One of the best experimental studies of the human memory yet made is that by Ebbinghaus Ueber das Gedächtnis, Leipzig, 1885. Summaries appear in James's Psychol., Vol. I, p. 676, and in the *Amer. Jour. of Psy.*, Vol. II, pp. 587-603. Ebbinghaus found that after a month

SMELL.

It was early suggested to me that the monkeys might be making their choices not through perception of color, shade, size or form, but through the sense of smell. Neurologically they would be expected to possess no such keenness of smell as the suggestion implies. Their olfactory lobes are said to show but very slightly greater development than those of the human subject, and certainly the human being under similar circumstances would have great difficulty in making such selections by means of the sense of smell.[1] Many indirect evidences have arisen which seem to indicate no such keenness. The male when offered some rice mixed with castor oil, took a handful of it, smelled of it at very close range and then threw it aside and wiped his hands. Here smell was a final test, but one made at very close range. When using two rectangular boxes, alike in every respect except that above the back side of one of them, a cardboard extended upward about three inches and bore various devices and was cut into different forms, although several hundred tests were made, food always being put in the same box, the male divided honors about equally between the two boxes. If in other cases where he was successful in making the distinctions, as with the forms and colors, he was dependent upon the sense of smell, then with these boxes he should also have been successful, for the stimulus in that event was the same here as in those cases. But if he was dependent upon the sense of sight, the distinguishing marks for the vessels being unlike in the two cases, the inference is natural that the monkeys simply failed to notice the designs and markings. Again when the feeding forms were glasses covered with colored papers, often when ten feet away from the board on which they stood, the monkey could be seen to give a quick glance along the board and then to make a bee line for the glass containing the food,—a type of behavior hardly capable of explanation on an olfactory basis. In some tests where the glasses were arranged in the order 1, 2, 3, 4, 5, 6, the monkey went to three, then looked into 4, 5, 6, 4, 3, and 2. The food was in 2, and 1 had just been used as the food glass 180 times, while the other glasses had never contained food. If the sense

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the same series of nonsense syllables could be relearned in four-fifths of the time originally required. The tests with the monkeys are not exactly comparable, but judging from what they did recall in ten tests I should infer that they would have relearned the maze after a period of fifty days in about one-third of the time originally required.

[1] The cases of Julia Brace, of the American School for the Blind, at Hartford, and others are exceptional, and are to be regarded as hypersensitiveness. Miss Brace is said to have been able to distribute the wash of the institution simply by the sense of smell.

of smell was the basis of choice this movement away from the food should not have continued all the way up to 6, and the glasses should not have been *looked* into with so great care.

When making the tests for discrimination of grays (second control test in the section on color discriminating) with the two glasses, both glasses had been used hundreds of times for feeding, yet where the shades were very different the monkey chose correctly almost every time. If he had been dependent upon odor for his choice he should have discriminated between glasses widely different no better than between those differing but very little in shade. But we have seen that choosing improved regularly as the difference between the shades was increased. In the experiments with designs where the shaded glasses were finally substituted for the designs the glass colored black had had food in it only a few times, yet the monkey sometimes chose black even though the food was in the lighter from which he had been fed many times, and my hand containing some food was nearer the food glass. Again in all the experiments with glasses and bottles the male only once put his nose to the neck of a bottle as if to smell for the food, and that may have been only an accidental position and no real effort to smell out the location of the food. The female never appealed in this way to the sense of smell.

These inferences and the general impression left upon the observer, both enforce the conclusion that the sense of smell was by no means acute in these animals, and that they relied in all of their choices to a vastly preponderating degree, if not exclusively, upon the sense of sight.

Individual Differences.

The individual differences between the two monkeys are very great. Many of them have been pointed out from time to time in former sections, and it only remains to draw them all together here.

The female is the older by some four or five months. She is larger and covered with more and longer hair. This is especially true of the face and brows, and gives her a more repulsive visage. The eyes and nose vary according to age as they do in the human family. With the young the bridge of the nose is low and broad and the eyes seem to stand far apart, but with the adult the nose becomes more prominent and the eyes seem to lie proportionally closer together. I received the female one month later than the male. This has made her appear at times less like the male than she really is: her earlier reactions to the laboratory environment being set side by side with his later ones. Dated notes along with tabular results, however, served to control this tendency.

The male is more timid than the female. I attribute it to the fact that he is less rugged than she. He has been ill twice, but has managed to regain his health. When ill he was unusually nervous. Owing to his nervousness he reacts quicker to any sudden noise, and " flies to pieces" before he has time to see the cause of the disturbance. When I approach the cage in which they are kept the female sits on the perch nearer the front of the cage. The male sits behind her or springs wildly about the back part of the cage. When out in the room together she will approach nearer than the male, and is bolder in her efforts to snatch food. The male is swifter in his movements than the female. When I desire to separate them I have only to open the door of the cage. He rushes out and there is plenty of time to close the door before she reaches it. When moving about the room from table to window, from window to sink and back to the large case or to the cage he is nearly always in the lead. When they climb a tree together he is in the lead both in going up and coming down. She does not run but shambles off in an awkward sidewise manner.

The interest shown in each other is worthy of mention. The male more frequently makes the request to be picked. If the female is put into the cage and taken out into the hallway he is much concerned and climbs the gas pipes and springs to the transom to see if she is near. Then he gives the call, in fact, repeats it often. He looks frightened and stops frequently to listen for her. But if *he* is taken out she settles down deliberately to business and shows no disturbance at his absence except occasionally to answer his calls.

In the earlier experiences she was the head of the family. But she exercised this family presidency on but few occasions, once with a significant stroke more vigorous than a caress, which, however, missed the mark, and again in a scramble for a vantage point in which he was worsted.

Since publishing the first part of this paper in which it was stated that the female was master, the male has begun to assert his rights. He now allows her to sit in front and reach for food, but then takes it from her, and pushes her off the perch. She makes no effort to regain the food, but patiently sits below and eats what he lets fall.

If several pieces of bread are tossed into the cage the male seizes one or two and proceeds to fill his cheek pouches. The female takes one in each hand and pulls others up near her as her own. I have seen her with one or two pieces near her and at the same time with a piece in each foot and a piece in each hand and another in her mouth. Her greed far surpasses his. She is more filthy in her habits than he, very frequently lapping her urine and even eating her excrement. He rarely

has done the former, and never the latter, though he certainly was as hungry as she. But neither has any idea or spirit of cleanliness.

In their reaction to the locks the male moved more rapidly about the boxes. He tried more persistently, and gave up only after many fruitless efforts. Thus on the horizontal hook he was less discouraged at the end of 17 minutes of trials than she at the end of 100 seconds. She required to be helped by having food placed on the parts to be moved, and did not at last substitute a better method of turning the lock after it had been learned. The male sometimes opened a lock a few times with his teeth and then substituted the hand. If the lock was tried with the hand and that failed the teeth were applied. He would try to work a fastening by moving it; she tried to chew it up or to break the strings with her teeth. She always seemed stupid in beginning, but often came suddenly to the idea, and finally, if it was an easy thing, learned it more quickly than he. Thus the first combination lock was learned by her in 80 trials, while the male required 253, but the amount of time required for the first manipulation, generally, is decidedly in favor of the male. With the second and more difficult combination lock she is far his inferior as the tables show. In all of the very difficult things he appeared to be superior, but for the easy things she was superior.

If we turn to the association with forms the reactions are very much alike. When the forms were set down the male sometimes rushed up quickly looking behind him, both right and left, as if frightened. He seemed scarcely to give more than a furtive glance at the forms. But the female shambled up in a slow wabble going straight to the point for which she started. After a series of experiments with the male, if there was perfect quiet, he became more composed and settled down strictly to business, but his nervousness and swiftness of movement will in part account for his greater irregularity after a thing was fairly well learned. Little noises sometimes veered him to right or left from his objective point. With the size boxes both showed a tendency to select larger rather than smaller boxes.

The female learned colors more readily than the male. For the most part this appeared to be due either to a clearer color perception or to greater associative capacity, but in part also to the fact that she approached more slowly, thus giving herself time to survey the line of glasses more carefully, while the male at times was so anxious and quick of movement that he rather took what he ran against. When he was inclined to do this I would hold the board until I saw him glance along it and then set it down.

When making these experiments it was necessary to take a minute to rearrange the glasses and put the food in. In the meantime the male would sit near by flea hunting or with his hands crossed, waiting for his next morsel. The female, however, would run to a distance and sit down. On coming again to the board she would walk or slowly shamble up, killing a great deal of time. Or, perchance, she would climb into one of the oak trees and get a dry leaf to eat. If the board were set down just as she got the leaf she would usually munch the whole of it before starting for the food in the glass. Sometimes, however, she carried the leaf with her.

As to color preference it has been shown that the male preferred yellow, orange and green, while the female fell to taking the food in regular order, and thus defeated the purpose of the experiment. When put into the box with colored illumination the male tried to escape, but the female spent her time in sitting about apparently in inquiry and wonder.

The individual differences in reaction to the number tests have been quite fully set forth in the section on number. In sum, it may be said that with the lower numbers she made the association more readily than he, but that he continued the associations up to six, while she stopped with three. In the reactions to the maze she required longer times at first and never went through in so short a time as he did, except in her second memory test, but she learned the maze with only about half as many trials as he required. The individual differences are apparently as great as they might be expected to be in two human beings selected at random. The assumption of some anthropologists that the lower animals of a given class react in practically the same way to the same environment can only hold with animals of this grade in the most general sense.

IMITATION.

In this section I want briefly to set forth what seems to me a reasonable conception of imitation and to bring forward the cases observed, to be evaluated according to the criteria presented, and this the more, because Dr. Thorndike, in experimenting with his monkeys, found no cases of imitation. If one declares, even after any amount of careful observation and experimentation, *without definition*, that an animal does not imitate, does not generalize, or does not reason, his declaration has on that account but little value. Again, if he defines these terms only in their highest forms of human activity, and then declares that the animal has not attained to them, his declaration has a certain value, but it gives only a limited view of the animal's activity. It merely states what the animal does not do. It amounts to a mere negation and nothing more. The pro-

cesses of imitation, generalization and reasoning, about which there is so much contention in animal psychology, all manifest themselves in the human being in more than one form. There are higher and lower forms. It is reasonable to suppose that animals will not present the highest forms, but that some of them will at least show something of the lower and preliminary forms. Such indeed is the necessary assumption, if we are to believe that the human mind is derived in unbroken series by increasing complication of factors found in the simplest animal reactions. Let us examine some of the classifications of imitative reactions.

Lloyd Morgan[1] classifies the imitative activities as instinctive and intelligent. Below these he places mimicry. On the latter point he says:[2] "Passing reference may here be made to those instinctive actions for which *mimicry* is now a recognized biological term. Certain distasteful butterflies, for example, are mimicked by others, which are believed to have escaped destruction because of their mimetic resemblance to the others. There is no intentional imitation. The mimicry is purely of objective significance. And not only in form, but also in their instinctive behavior, are many of these insects, and perhaps some birds, mimetic of others. Such behavior is, from the purely objective point of view, imitative. But since there does not seem to be any good ground for supposing that the mimetic behavior is called forth by the stimulus of such behavior in the models, it does not fall under the head of instinctive imitation we are considering. By using the term 'mimetic' in its biological signification, we may mark off these cases in mimicry in behavior from true examples of instinctive imitation—that is to say, instinctive behavior called forth by similar behavior in others."

By *instinctive imitation* is to be understood "the congenitally automatic behavior, which from the observer's standpoint, is imitative." "It is an organic response independent of experience." This phase appears to be identical with Baldwin's organic imitation.[3] In such cases there stands an instinct ready to be called into action by its appropriate stimulus. The newly hatched chick will give a warning signal at the sight of some threatening object. His action is purely instinctive. Another chick, not seeing the object, will take up the refrain and will repeat the signal. Thus, though the usual or perhaps appropriate stimulus for setting off that instinctive action, with the second chick, is wanting it takes the warning

[1] Habit and Instinct, p. 174.
[2] Habit and Instinct, p. 169.
[3] Mental Development, p. 263.

signal of the other chick as its cue. Hence, from the observer's point of view, chick No. 2 imitates chick No. 1, but in reality his behavior was only an automatic response. For further illustrations of this phase of imitation see Lloyd Morgan's Habit and Instinct, p. 166. Apparently the instance presented by Dr. Thorndike, Animal Intelligence, p. 48, can be explained in this way. The first members of a flock of sheep, as they were driven along, were compelled to jump over a hurdle. Then the hurdle was removed, but several sheep immediately following sprang up as if to clear the bar. The sight of the leap in one, induced reflexively, similar action in the one succeeding. Objectively it looks like imitation, but subjectively it is automatic behavior.

True imitation implies an action or a result to be copied, and a certain amount of preliminary experience, is due to conscious guidance, and is based upon the immediate satisfaction which accompanies the act of imitation itself. When a child is to trace a curve the "preliminary experience" consists in the previously "acquired data in the light of which control over his arm and finger movements may be exercised." "In the case of the curve, the child first imitates the action—holds the pencil and moves the fingers in certain definite ways. But as soon as passable results are reached, it is on this, and not on the movements, that he fixes his attention. His object is no longer to imitate the action, so much as to reproduce the copy."

Again, imitations have been classed as simple and persistent.[1] "By *simple imitations*, reactions are characterized, in which the movement does not really imitate, but is the best the child can do. He does not try to improve, by making a second attempt." By *persistent imitation* is meant the child's effort, by repetition, to improve his imitations.

In a word then we have:—

1. *Mimicry*, which lies below the level of imitation.
2. *Instinctive imitation*, or automatic behavior.
3. Intelligent imitation.
   a. Of actions.
   b. Of results.

Two special forms have been described:—

(1) *Simple*, consisting of single efforts, but with no succeeding attempts with a view to improvement.

(2) *Persistent*, consisting of several attempts at improvement.

If I am asked whether these two monkeys have imitated, the answer must be divided. Corresponding to the *mimicry*, described above, we have the instinctive crouching, simulating

[1] Baldwin's Mental Development, p. 132.

the crouching of the feline tribes of animals. (See General Observations.) So far as these animals are concerned they spring upon nothing. The crouching simulates the action of an animal which does crouch and spring. But the crouching is used by these only as a bluff, and certainly with no notion of imitating those animals or its fellows. And indeed it does appear threatening, but a wave of the hand or a square look into his face changes bravery into precipitate flight. I am not informed as to whether the adult *rhesus* ever crouches and actually springs upon his prey, but these young ones have shown no sign of doing so.

Instinctive imitation, or automatic behavior, was observed frequently when the monkeys were in separate cages. When one could not see the other, if either gave the danger signal, or the food signal, the other immediately repeated it. The similarity to the action of the chicks described by Morgan is so close that no further discussion is necessary.

Again, of the simple form as discussed by Baldwin, I observed one very clear case. The male looked under the bottom of one of the trees that stood in the room. This required that he should put his face clear down onto the floor. Immediately afterwards the female took a peep in exactly the same way. She did this but once. This appears to me to come clearly under Morgan's class of intelligent imitation of an act. Of course the female did not set out deliberately to repeat the act simply for the sake of the act. But seeing that he did thus and so she voluntarily and not reflexively repeated the act to satisfy her curiosity or in the hope of getting food, though the male had gotten no food as a reward for his act. There were many other cases of this type, as it seems to me, though they are more difficult to demonstrate. The difficulty arises out of the fact that the actions may have complex motives. As, for example, if the male springs upon the table or into a window, or climbs a tree, the female follows suit. Does she imitate his act? Possibly she does. Possibly she is actuated by the same motives as he, and being the slower of movement is a little behind him. This gives her the appearance of imitating. My belief is that having once clearly demonstrated that she could imitate, we are safe when evaluating such cases as the above in assuming that she responds to a complex motive of which imitation is probably a part. Some of the other motives are fright, feeding and flight, and the social instincts.

But again I have observed two cases of imitation of the *persistent* and *intelligent* types. These have already been described at length. (See Simple Locks, Methods of Learning.) Here we have a copy in the form of an act. It was copied almost in detail, and that, too, so far as the place of laying

hold of the plug and the direction of the pull were concerned, both requiring very radical changes from the monkeys' own previous efforts. Further, the copy was repeatedly followed and fashioned into a well-defined habit.

Then, have the monkeys imitated? Neither have imitated any of my acts so far as I am aware. The male has rarely ever done anything that could be regarded as an imitation of the actions of the female. The female, however, has imitated the male, manifesting every phase of the process as defined above.

General Notions.

I can do no better here than to follow the general plan of the preceding section.

Romanes[1] used the word *Idea* as a generic term to signify, "indifferently, any product of imagination, from the mere memory of a sensuous impression up to the result of the most abstruse generalization."

"By *Simple Idea*, *Particular Idea* or *Concrete Idea*, I understand the mere memory of a particular sensuous perception.

"By *Compound Idea*, *Complex Idea*, or *Mixed Idea*, I understand the combination of simple, particular, or concrete ideas into that kind of composite idea which is possible without the aid of language. [Called by Romanes a recept.]

"Lastly, by *General Idea*, *Abstract Idea*, *Concept*, or *Notion*, I understand that kind of composite idea which is rendered possible only by the aid of language, or by the process of naming abstractions as abstractions."

One may not be inclined to accept the name here applied by Romanes, yet psychologists accept some such phenomena in mental life as the basis of naming. (Ribot and James.) The particular images, or ideas, are of the nature of mental images, or memories of such and such objects, as the sound of a voice or the image of a particular horse. If there is such a thing as a general idea, it must arise out of particular ideas, as when we get the general idea, horse, from observing numerous horses. His second and third classes above would include general ideas. The third class (barring discussion of the problems of nominalism, idealism, and the notion of the "rule" or "schema" as designated by the general terms of language) represents the higher human generalizations.

Ribot names three classes of general notions above and beyond the pure individual representations, the first named by Romanes.

1. *Abstraction and generalization* with no possible aid from

[1] Mental Evolution in Man, 1899, p. 34.

language. These are called *generic images* by Huxley, Galton and Ribot. They are the same as Romanes's "recepts," and are intermediate between the pure image on one hand the generalizations on the other.[1]

2. *Intermediate abstraction*. These imply the use of words. "At their lowest stage they can hardly rise above the level of the generic image; they can be reduced to a vague schema, in which the word is almost a superfluous accompaniment. At a stage higher the parts are inverted; the representative schema becomes more and more impoverished, and is obliterated by the word, which rises in consciousness to the first rank."

3. *Higher Concepts*. These can no longer be imaged. This is the characteristically human form where everything is subordinated to language.[2]

In brief form we have then:

1. Individual representations, concrete ideas, pure images. This class is offered not as a kind of generic image, but as lying just below the level of generic images, and yet closely allied to them.

2. Generic images, or recepts, compound idea, complex idea, mixed idea, abstraction and generalization with no possible aid from language.

3. Intermediate abstraction.

4. Higher concepts, general idea, abstract idea, concept, notion.

Accepting the above classification we are ready to ask whether the monkeys have given any unmistakable signs of having any of these classes of ideas.

It may be doubtful whether the monkeys have "free ideas," that is whether they do call up the image of anything not now present to the senses. Do their minds as they sit quietly on their perch play with images and scenes from other times and places? It is probably impossible to know that they either do or do not have such images. Those committed to the law of parsimony, can say in truth, that it is possible at least to reduce all seeming manifestations of such free images to lower terms; while those not so committed can say with equal justice that the same may be said of human behavior also, and with the monkeys we observe such objective phenomena as in the human being are the regular accompaniments of free ideas. Such acts as the efforts to enter the box for food, smacking the lips while passing through the maze, climbing the gas pipes and springing to the transom to see the mate that had been caged and removed to the hall, and other similar acts are, to say the

[1] See, also, James, Vol. II, p. 46-48.
[2] Evolution of General Ideas, pp. 10 and 88.

least, very like what human beings do when calling up images of objects and persons not present to the senses. These are fairly good signs of our first class, particular images.

As to the generic images not requiring the use of language it seems to me that three very favorable cases have arisen, two of which bear some of the ear-marks of the still higher form regarded by Ribot as *intermediate abstraction*. The first case is that presented in the third section of Methods of Learning. It seems to me that the only reasonable way to explain how the monkey came at last to single out and attack a fastener at a new place on the door, immediately on coming up to it, while in the earlier tests he tried almost anywhere, is to say that he developed some sort of a general notion of a localized hindrance. The fact of projection above the surface of the box would appear to be the only general quality of these fasteners, though the plugs and levers had more in common than all the other fasteners had, such as being made of wood, projecting rather far, having a rigid appearance, etc. It seems quite probable at least that some such notion arose and served as a starting point in singling out from the total complex of a box with door, holes, edges, lights and shadows the objective point for attack. No control tests were made, however, to verify such a general notion in this case.

The case described under the second control test in discrimination of color and shade seems to be a fairly good one and represents a tolerably high degree of abstraction. The generic image, if such it was, was that of a darker and a lighter glass with food in the lighter. This image was then modified to fit the new conditions whatever they might be. There was one clear alternative, namely: the monkey's æsthetic taste, or an instinctive feeding preference for lighter over darker forms, may have been so strong as to draw him always to the lighter forms. But the tests with designs, where the food was put into the darker glass, contradict such a supposition. I undertook by the same method as that employed in the tests with designs to change the male's feeding habit to the darker glass. But the old habit established by 3,200 tests proved to be so strong that it would have required several hundred tests to change it. So I used the female. Four hundred tests were made with her, feeding in the darker glass. These sufficed to show clearly that she would establish as strong a feeding habit, if not a stronger, with the darker as the male did with the lighter form.

Still a third case was noticed, and is reported under the fourth control test on discriminations of color and shade. Referring to the tables there we see that when the blue glass with its three corresponding grays was presented, the male in the first thirty chose correctly 17 times, and the female 13 times. When

the yellow was next presented with its grays the correct choices in the first thirty tests were 12 for the male and 27 for the female. With the red the figures are, 26 for the male, 24 for the female. When the green was reached the corresponding numbers rose to 28 and 30 respectively. This difference becomes more striking if we compare the first ten tests of each color instead of the first thirty, since it shows better how abruptly the correct choosing came in. The correct choices in the first ten tests of the male, for the blue, yellow, red and green respectively were, 2, 2, 8 and 8; the female's were, 5, 8, 8 and 10. Why this improvement as we change from one color to another? Is it because the green and its grays are distinguished better than the blue and its grays? The next table, the tests for which were made some days later, would indicate no such difference in favor of the green. Besides now the blue is chosen out from among its grays, practically as well as is the green from its grays, and the choices of yellow with the male have risen now to an equality with the others.

Then do we have here a general notion to be represented perhaps by "food-always-in-the-odd-glass?" If so we might substitute one red and three blues or a gray of one shade and three grays of another shade and still get from twenty-five to thirty correct choices out of thirty tests. But when this was done they immediately dropped to from three to ten correct choices out of thirty. Clearly, then, this was not the general notion in the mind of the monkey. But suppose we should say that the notion was such as could be represented by the formula "food-in-the-colored-glass." This seems more plausible. The notion has risen from extreme vagueness to almost perfect clearness. Now the reaction to all colored glasses or to all grays as before is all confusion and is just what we might expect from an adult human being if we were to place three blue and one red, or three dark and one light gray glasses in a room and tell him to bring us the colored one. He would look them over several times and then bring all or none.

But there are yet two possible alternatives for avoiding the idea of a general notion here. The monkeys had been fed from colored glasses on this board before; they then would be expected to go to the colored glasses instead of the grays. But they were fed from grays when the six grays were used in the control test, and that came after the feeding from colored glasses. Besides this the male started out in the first of these tests going to the grays five times as often as to the blue though there were only three times as many grays as blues. The female, however, went at once to the blue half as often as to the grays. Clearly that alternative will not explain the readiness for selecting the colored glasses as the ones most liable to contain

food. The other alternative is to say that the colors lay nearer the feeding instinct of the animals than grays or that their æsthetic tastes would draw them to these colors in preference to the grays. But that does not account for the improvement. Something gained in the first and second of the tests (with blue and yellow) must have been carried over to the third and fourth, or else the monkeys would have been under the necessity of building up an association with the green and its grays just the same as with the blue and its grays. What was it that was gotten and carried over? The law of parsimony is a valuable law, but in the face of these facts one can hardly avoid conceding to the monkey something like a generic image, by means of which he was able to carry over to the new situation something of his previous experience. To do less is to shoot below the mark.

No one, unless it be Mr. Garner, and he need hardly be reckoned with as a psychologist, and Romanes, perchance, would ascribe to the monkey any of the more general ideas requiring the use of language.

Then to answer our first question in brief: (1) These monkeys appear to have had individual representations of percepts —that is, *A* being perceived they imaged *B*, though *B* was not then present to their senses. (2) They very probably have generic images. (3) The intermediate abstractions, with merely positions of the body, or calls as their signs, may have been present. (4) Intermediate abstractions and higher concepts, both requiring the use of language, are wholly wanting.

I think it safe to say with Ribot that the faculty of abstracting, from the lowest to the highest degrees, is constantly the same; its development is dependent on that of (general) intelligence and of language; but it exists in embryo even in those primitive operations which are properly concerned with the concrete, *i. e.*, perception and representation. Höffding takes the same view.

REASON.

Have these animals reasoned at all while under observation? No simple answer can be given. As before, the question turns upon what one means by reasoning. I find Commissioner W. T. Harris, describing what he calls the Logic of Sense Perception. DeGarmo has reiterated the same in his Essentials of Method. My recognition that yonder object is a horse implies something like this:—

Minor premise. This object has such and such characteristics.

Major premise. Horses have these characteristics.

Therefore . . . .

The syllogistic *forms* in cases of perception are really entirely wanting. From a psychological point of view they are not to be reckoned with, though *logical analysis* may find such processes implied. Where the process of perception is very slow, recognition, identification and verification, the implied logical forms, may rise very nearly to explicit logical forms. But while such analysis may have pedagogical significance, it has no psychological importance, and the matter is here mentioned only to be excluded. They are at most but cases of *implied reasoning*.

Unconscious or immediate inference ranks next in order. A good description of it is found in James.[1] It occurs "where a present sign suggests an unseen, distant, or future reality. Where the sign and what it suggests are both *concretes which have been coupled together on previous occasions*, the inference is common to both brutes and men, being really nothing more than association by contiguity. A and B, dinner-bell and dinner, have been experienced in immediate succession. Hence A no sooner falls upon the sense than B is anticipated, and steps are taken to meet it. The whole education of our domestic beasts, all the cunning added by age and experience to wild ones, and the greater part of our human knowingness consists in the ability to make a mass of inferences of this simplest sort. Our 'perceptions,' or recognitions of what objects are before us, are inferences of this kind. We feel a patch of color, and we say 'a distant house;' a whiff of odor crosses us, and we say 'a skunk;' a faint sound is heard, and we call it a railroad train. Examples are needless; for such inferences of sensations not presented form the staple and tissue of our perceptive life, and our Chapter XIX was full of them, illusory or veracious. They have been called unconscious inferences. Certainly we are commonly unconscious that we are inferring at all. The sign and signified melt into what seems to us the object of a single pulse of thought. Immediate inferences would be a good name for these simple acts of reasoning requiring but two terms, were it not that formal logic has already appropriated the expression for a more technical use."

Ribot describes what appears to be the same as *immediate inference* under the caption, "Inference from particular to particular," under *Logic of Images*. His other subdivision is "Procedure by analogy."

"1. *Inference from particular to particular*. The bird which finds bread upon the window, one morning, comes back next day at the same hour, finds it again, and continues to come.

[1] James, Vol. II, p. 326.

It is moved by an association of images, plus the state of awaiting, of anticipation as described above.

"2. *Procedure by analogy.* This (at least in its higher forms in animal intelligence) presupposes mental construction; the aim is definite, and means to attain it are invented. To this type I should refer the cases cited above of ants digging tunnels, forming bridges, etc. The ants are wont to practice these operations in their normal life; their virtue lies in the power of disassociation from their habitual conditions, from their familiar ant-heap, and of adaptation to new and unknown cases."[1]

It seems to me that there should be recognized a phase of reasoning somewhat lower than the preceding, and in a general way very like that described by James as "rational thinking," in that it consists of a train of images, but different in that it is connected immediately with perception and action. To illustrate, one desires to reach a certain point on the side of his house. He stands near by and looks over the situation. He now sees that an attempt to go up over the window would fail. He looks at his step-ladder near by, but that is too short. But he notes that he can get on to a low roof from this ladder, pass along to the right and reach the desired point. Free this process from the immediate presentations and we have James's "rational thinking," which in its more purposive forms becomes the predominant process of the inventor. But so long as it is mainly for purposes of action and consists in thinking of one's self in new attitudes and positions within the perceptual field it may with propriety be designated as *adaptive intelligence*, and is of about the same grade of rationality as analogical reasoning which has not yet reached full consciousness and verbal expression.

James's description of "rational thinking" is brief, and I quote it in full.

"Much of our thinking consists of trains of images one suggested by another, or a sort of spontaneous revery of which it seems likely enough that the higher brutes should be capable. This sort of thinking leads nevertheless to rational conclusions, both practical and theoretical. The links between the terms are either 'contiguity' or 'similarity,' and with a mixture of both of these things we can hardly be incoherent. As a rule, in this sort of irresponsible thinking, the terms which fall to be coupled together are empirical concretes, not abstractions. A sunset may call up the vessel's deck from which I saw one last summer, the companions of my voyage, my arrival into port, etc.; or it may make me think of solar myths, of Hercules's and Hector's funeral pyres, of Homer and whether he could

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[1] Evolution of General Ideas, p. 26.

write, of the Greek alphabet, etc. If habitual contiguities predominate we have a prosaic mind; if rare contiguities, or similarities, have free play, we call the person fanciful, poetic or witty. But the thought as a rule is of matters taken in their entirety. Having been thinking of one, we find later that we are thinking of another, to which we have been lifted along, we hardly know how. If an abstract quality figures in the procession, it arrests our attention but for a moment, and fades into something else, and is never very abstract. Thus, in thinking of the sun-myths, we may have a gleam of admiration at the gracefulness of the primitive human mind, or a moment of disgust at the narrowness of modern interpreters. But, in the main, we think less of qualities than of whole things, real or possible, just as we may experience them.

"The upshot of it may be that we are reminded of some practical duty; we write a letter to a friend abroad, or we take down the lexicon and study our Greek lesson. Our thought is rational, and leads to a rational act, but it can hardly be called reasoning in a strict sense of the term."[1]

Finally there is the more formal type which consists in associating two ideas through the mediation of a third.[2] It is the "substitution of parts and their implications or consequences for wholes."

The pure *logic* of reasoning must, however, be kept distinct from the practical or psychological process. The latter involves, according to James (Vol. II, p. 340), first an act of sagacity in extracting from a total complex presentation some character which is then taken as an equivalent to the entire datum from which it comes, and second an advance from this to certain suggested consequences more obviously seen in it than in the total datum as originally presented.

Enumerating briefly we have:—

1. Implicit reasoning. (Harris.)
2. Inference from particular to particular. (Ribot.) Unconscious or immediate inference. (James.)
3. Adaptive intelligence.
4. Analogy.
5. Rational thinking. (James.)
6. Formal reasoning.

The first is present with these animals quite as much as with human beings. There can be no doubt that the monkeys recognize kinds of food and other objects of familiar classes, and it is as fair to apply logical analysis to this process in them as to like processes in human beings. But this is not, from the stand-

[1] James, *op. cit.*, Vol. I, p. 325.
[2] James, *op. cit.*, Vol. II, p. 330.

point of psychology, in any sense a reasoning process either in men or animals.

Cases of reasoning by immediate inference, as James calls it, are numerous. When they saw the box used in experimenting, and although they had just then seen no food put into it, the sight of the box suggested the idea of food and the box was opened. When in the maze certain parts well on toward the end suggested the food and the lips were smacked. Seeing me the food signals were given. Seeing the sink suggests the idea of water in a pan just out of sight, and the sink is climbed for water. Here we have Ribot's "state of expectation," an anticipation, but it is expectation based on the fact that in a previous experience the present percept was followed by the thing now expected to follow. It is a case in which we have two terms associated, but have the mind occupied less with the memory of the past than with the expectation of the term to follow.

The adaptive intelligence, which is hardly reasoning, was well illustrated on one occasion when I tied a piece of apple to the tip end of a limb of one of the trees. This was a strong, long limb, the last four or five feet of which extended along the ceiling. The monkey's weight on this limb could not bend it down. He watched me as I tied the apple fast. Then he rushed up the tree nearly to the place where it brushed the ceiling. He was baffled, and returned part way down the tree, after which he passed out another limb and looked the field over for a full minute. The position was changed and the field scanned over again. Then he went up the tree to the ceiling, swung himself under the limb and went along it hand-over-hand until the apple could be reached. Here the thinking is that of tracing out a line of conduct, planning a campaign and then executing it. It probably consists in putting in consecutive order, somewhat imperfectly, a series of percepts as a guide to future activity. One might wonder whether he did not in his joints, tendons and hands feel himself making the journey. This looks very much like a capability for analyzing a situation. So does the case already reported where the monkey sprang from the corner of the table to the large post and grabbed a pear as he went. But such a capacity apparently neither Mr. Thorndike nor Mr. Morgan will grant.[1]

Whether their processes rise to the fourth level becomes a more difficult problem. Do these monkeys recognize similarity? Do they, in working the latches for example, have a dim consciousness that this projection is like the one whose pulling gave them food, and then come to feel that pulling this

[1] Nature, July, 1898, p. 249.

ought to do the same thing. This, of course, we cannot positively determine. Earlier in the paper and again in the preceding section it was reported that the monkey in his first tests on locked-boxes attacked no specific part of the box, and later invariably attacked the fastener even though it was of a new form and at a new place. Thus, when the push bar was put into the right side of the box no other fastener had been put on the box near where it was placed, except a plug, for several series of tests preceding. It had been preceded by a string, ring and nail on the front, by a latch at the left and front, and by a string in the rear of the box. But when the box with the new latch was set before him he rushed by the front and attacked the pusher immediately. One of two things, he either recognized the similarity of this pusher to other fasteners though it was similar to the others only in being a projecting thing, and to the plugs, levers and lift-latches in being a wooden projection, and concluded that working it would bring food; or, the pull at that point was a mere reflex from the sight of the projecting latch itself, a sort of acquired tropism by which he was attracted to the latches in general. With the first four fastenings the monkey began and opened each several times by mere accident. It took time to discover the latches themselves. But when the fifth was presented, the door bolt, it was attacked directly and moved in one second. Thereafter, with a single exception, the locks were attacked directly and no energy or time was wasted in trials anywhere else about the box.

I am inclined to think that in such cases as this there is a complex state of mind, involving partly the reflex from the sight of the object, and partly the recognition of similarity, along with the food idea, and possibly several other minor factors.

Morgan and Thorndike both insist that animals cannot reason by analogy. However true this may be of chicks, cats and dogs, I very much doubt whether it is true of *rhesus* monkeys. The ruling out of reasoning by analogy with all lower animals, it seems to me, is often due to a failure to differentiate sufficiently the psychological process of analogical reasoning, resulting in practical activity, from a subsequent logical analysis, accounting for the intelligent act. Of course the animals cannot do the latter. In part their reasoning is like that of the human being. Yesterday a man saw a vine and handled it without evil results. To-day he sees another quite like it, handles it and is poisoned. He does not say "Lo, now, here is this and this likeness, therefore it is safe to handle this vine." He was just dimly conscious of a resemblance. He may not possibly be able to name a single likeness if put to the test. So far in his process he and the monkey have gone along together. But

just here they part company. You ask the man why he handled this poisonous vine. Now he furnishes you a logical analysis, a reasoning by analogy, a subsequent explanation of his conduct, more or less definite according to his powers of observation. He says, "Why the thing looked just like the vine I handled the day before, and I never thought of its poisoning me, or I thought it would not poison me." In other words he gives explicitly what was implied in his previous act. From the standpoint of logic he reasons analogically in a process parallel with the analogy implied in his previous act.

Mr. Morgan makes this distinction very clearly in his Introduction to Comparative Psychology, pp. 279-283. He regards what I have here called the psychological process, as "a sequence of an impression, and an idea in sense-experience without implying either perception or conception," and calls it *intelligent inference* in the field of sense-experience. He, apparently, would regard the process as of a kind with our (2) and (3) above (Inference from particular to particular, and Adaptive intelligence). Further, he would say that the logical process is in no wise implied in the psychological, but only the relations on which the logical is afterwards based. He says, by way of conclusion, that he wishes to use the word "intelligence" for the faculty in virtue of which inferences are suggested in the field of sense-experience, and "reason," where the logical relation is *clearly* perceived. Every one, I presume, would grant at once that most cases of apparant implied analogical reasoning, may be nothing more than association, immediate inference or adaptive intelligence; but my position is that some cases, such as that of the latches, where the "prophetic" phase is present quite prominently, may come under Mr. Morgan's implied category of *less clearly perceived*, and yet be reasoning by analogy. The real question is, of course, that of fixing the degree of "clearness" required for analogical reasoning. As a matter of fact the lower forms pass into the higher by imperceptible gradations not to be fixed by hard and fast definitions.

All of the lower forms of reasoning, and forms of activity antecedent to reasoning are usually granted to the lower animals, unless it be that of James's rational thinking, which is hardly to be reckoned as reasoning, but more nearly perhaps as reverie. The field of greatest contention is at the level of analogical reasoning. As to the still higher forms of human reasoning it has often been asserted that animals have employed them, but never, so far as I know, by any psychologist of acknowledged standing.

RÉSUMÉ.

We have seen that these monkeys have indulged in movements signifying emotions such as fright, anger, social feeling, disappointment, curiosity and surprise. The emotional range has been very limited however. There has been no friendly spirit manifested towards the keeper or towards any one else. But the desire for the company of their fellows has been shown frequently by the loud calls when they were separated. There has been no mischievousness nor destructiveness simply for the sake of those activities. Nothing has been destroyed or injured except in attempts to escape and to procure food. The instinctive fear of snakes and cats is present. But frogs and newts merely surprised. There has been no manifestation of play such as springing about the cage or running after each other in sport. The mutual flea-hunt is their only amusement, if indeed it can be called such. Several sounds have been used signifying food, danger, loneliness, anger and disappointment, but these are not words, only very general instinctive responses.

The monkeys have learned to manipulate simple and complex locks and allied apparatus. In doing so they have made a little progress in capability for choosing better methods of working, inhibiting useless acts, and employing short circuiting processes. We have seen how the formation of a third association of three similar but opposing ones resulted in a partial recall of the first, and that it is more difficult to break one association and then form a new, than to form one *de novo*. (See experiments with forms.) We have found several cases of imitation, one being of very high order. We have found evidence, also, of general notions and reasoning, both of a low order. It has been shown that the monkeys associate food with forms more readily than with sizes and colors. In the choosing of sizes they erred in favor of larger forms, whether the sizes were arranged in an arithmetical or geometrical progression. They certainly are able to distinguish colors as colors and not merely as shades of gray. As regards color-preference it can be said at most only that it is probable that they like yellow, orange and green better than red, purple, blue and gray. They were able to do no real counting, but position in a series of objects was recognized by one as far as 3 and by the other as far as 6. Their general intelligence appears to be high, if we compare the time curves determined by Mr. Thorndike with cats, dogs and chicks. Human beings have not greatly surpassed them in some respects when dealing with the apparatus here used. In many of the experiments we have reported the errors made by the animals. It often happens that the errors are more important in the animals' processes than are the time results.

The sort of errors made, the elimination of many unnecessary movements and the character of many other reactions not classified here incline the writer to believe that the monkey's mental processes are after all not so simple as analysts have often asserted them to be. Whether these animals have "free ideas" and general notions beyond the mere 'recept,' and are capable of real analogical reasoning cannot be positively determined. If they do, the processes certainly do not rise to the level of full reflective consciousness. Yet there is no way of knowing, because there is no certain way of having the consciousness that the animal has. But that these monkeys have often acted objectively just as human beings act when they have these mental activities is most certain. I am inclined to believe that the human and animal consciousnesses are not really different in kind but only in degree; the difference in degree, however, is very great.

APPENDIX.

It is my design to furnish in this section a brief study of the *Macacus rhesus* monkey such as I have been able to extract from the literature of the subject.

The Bhunder or *Macacus rhesus* is the Bandar of the Hindus, and is to be found all over Northern India. It is a very strong looking animal when full grown. The body of the adult is from fifty to sixty-five centimeters long. The tail is about one-third as long as the body. The body is large and thick in front and tapers backward. The limbs and shoulders are very strong. The prevailing color of the hair is olive-green and brown on the back, and a dull white underneath. The face, ears, hands and seat-pads vary with age from a pale flesh-color to a light copper color. With age the face and callosities often become very red.

The whole appearance is so baboon-like that Cuvier,[1] Shaw and Audibert classified them as baboons. In Cassell's Natural History, edited by P. Martin Duncan, New York, 1884, they are classified as dog-shaped monkeys, while Brehm classes them in the same family with the Dril and Mandril, neither of which is ever classed as anything except a baboon. The Royal Natural History, edited by Richard Lydekker, London and New York, 1893-94, Vol. I, p. 113, discusses the Bhunder (*Macacus rhesus*) under the heading, "The Bengal Monkey."

While they are thus classed as one of the lowest of the monkeys or as a good baboon they are invariably regarded as having a high degree of intelligence.

They inhabit a great portion of the inlands of India, and are

[1] The Animal Kingdom, Cuvier, London, 1827. Vol. I, p. 289.

found continuously northward from the valley of the Godaver to the Himalaya, extending to the west coast at Bombay. Great numbers live in the valley of the Ganges, and some are to be met with in the warmer valleys of the Himalayas. They have been seen in the neighborhood of Kasmir at an elevation of 4,000 feet, and near the sanitarium of Simla at an elevation between 8 or 9,000 feet above sea-level. Hutton reports that he saw these monkeys there in February when the snow was from ten to fifteen centimeters deep. They slept at night upon the trees and were apparently oblivious to the cold. They were, however, more stupid than when the weather was warmer. The monkeys were more abundant in winter than in summer in the vicinity of Simla. At times they were seen springing and playing under the pine trees whose boughs were covered with snow.

Near Simla is a hill called Jako. A fakir lives here who regularly calls the monkeys and feeds them. No one has properly done Simla who has not walked or ridden out to see the monkeys fed.

In Bengal the Bhunder lives in great numbers. They like the forests, where they are invariably found. Usually they rove about in great droves. They often go into the plains and cultivated fields, and not unfrequently, even into the towns and cities. Crooke[1] says "The *rhesus* is a most troublesome, mischievous beast, and does enormous mischief to crops, while in cities he is little short of a pest. But his life is protected, by a most effective sanction, and no one dares to injure him." These monkeys are probably never regarded as sacred, though they are protected, and tithes are left in the fields for their benefit. In places they form "part and parcel of the appendages of the temples." There is strong objection to killing these monkeys, probably partly due to the general belief that no one can live where a monkey has been killed. A monkey's bones are exceedingly unlucky.

They delight in the small streams, and are good swimmers and can dive successfully, apparently coming out at the landing previously chosen. When wild these monkeys make a hideous noise with their calling and chattering. They eat large quantities of fruits and seeds, and do a great deal of digging for insects and spiders.

They are docile and easily taught if captured and trained while young, and are much used by fakirs and in shows. They are favorites with trainers for they learn their tricks easily and work at them with great endurance and skill. In captivity, in

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[1] Popular Religion and Folk-Lore of Northern India, 1896, Vol. I, p. 88.

their happier moods, they join with their fellows in the "flea-hunt," but are liable to fall out at any time and go on the war path. In youth they may be affectionate toward their fellows and their keepers, but as adults they occasionally like their fellows but always hate human beings. The trainer may make them submissive, but they cannot drive all of the treachery out of them. When they go into a rage they break, tear up or destroy whatever they can lay hands on. If free to do so at such times they will attack a man fearlessly, using their powerful teeth with great skill and force. They are constantly threatening and bluffing at every one who approaches near the cage. They open their mouths, the eyes bat and they set their bodies into a crouching position like that of beasts of prey. Then they whet the teeth, puff out the cheeks and bob part way up as if just starting to spring upon one. Yet unless greatly annoyed they would in reality run away if they had half a chance. Lydekker reports an instance where a gentleman came across a party of these monkeys, among whom were several females with young ones. He undertook to run them down in order to capture one of the young. The old males deliberately charged upon him, and he was compelled to shoot the leader in order to make good his escape.

In captivity they are ill-tempered, jealous, selfish and tyrannical. When males are caged together they are very quarrelsome, so keepers prefer to cage a male and female together.

Lydekker repeats a story from Prof. Bell, who says, "When at Malwa Tal (near the Himalayan Station of Naini Tal), which is one of the lakes where I spent a day, I was warned that, in passing under a landslip which slopes down to the lake, I should be liable to have stones thrown at me by the monkeys. Regarding this as being possibly a traveller's tale, I made a particular point of going to the spot in order to see what could have given rise to it. As I approached the base of the landslip on the north side of the lake, I saw a number of brown monkeys (*M. rhesus*) rush to the sides and across the top of the slip, and presently pieces of loosened stone and shale came tumbling down near where I stood. I fully satisfied myself that this was not merely accidental; for I distinctly saw one monkey industriously, with both forepaws, and with obvious malice of pretense, pushing the loose shingle off a shoulder of rock. I then tried the effect of throwing stones at them, and this made them quite angry, and the number of fragments which they then set rolling was speedily doubled. This, though it does not actually amount to throwing or projecting an object by monkeys as a means of offence, comes very near to the same thing, and makes me think that there may be truth in the stories of their throwing fruit at people from trees."

In some parts of India they are left very much to themselves; so they assemble in troops, and steal from among the natives in a very troublesome manner.

As they are bold, their habits in the wild state are often observable, their slyness and thieving propensities being most amusing. They gather on the roofs of the low houses in the bazaars, and look out for occasion to steal.[1]

In conclusion, I translate from Brehm, p. 134, Cuvier's report of his observations of a mother Bhunder and her baby born in captivity.

"Immediately after birth the young *rhesus* climbed upon the belly of the mother, meanwhile he held firmly to the fur with his four hands, and took the nipple in his mouth, and did not release it for fifteen days. He remained during the entire time in an unchanged position, always engaged in sleeping and sucking. He released one nipple only that he might seize the other. And so he passed the first days of his life making motions only with his lips for sucking and his eyes for seeing. Like all monkeys he was born with his eyes open, and it appeared that from the first moment he could distinguish his environment, for he followed all moving objects with movements of the eyes.

"It cannot be reported, how great the anxiety of the mother was, and how much she was concerned with the sucking and security of her new-born baby. She showed herself often so intelligent and cautious that one wondered at her. The least noise, the gentlest movement eliciting her attention, and anxious care for her young, not for herself; for she had lived with men and had become perfectly tame. All her motions took place with the greatest dexterity, and yet never in such a way that the suckling suffered any. The weight of the young appeared to hinder none of the mother's movements, and made no difference in her activity and bluster. However, it was significant that the mother took double care not to strike her young against anything. After about fourteen days the young began to release himself from his mother and to run about, showing an astonishing strength and dexterity considering that he had had neither previous practice nor experience. The young *rhesus* clambered at once upon the vertical wires of his cage, climbed on them as he would, up and down, took a few steps upon the straw, sprang freely from the top of his cage down onto his four hands and then against the grating onto which he climbed with an agility and security, which would have done honor to the most skillful monkey. The mother followed each movement of her young with the closest attention and appeared always ready

[1] Cassell's Natural History, p. 121.

to prevent any possible injury to him. Later she sought from time to time deliverance from her burden but always showed care for him, and if she thought him in jeopardy she prepared to suffer for him. Also upon the slightest touch of him with her hand the tractable pupil would turn back, and he took quickly the position at the breast of the mother. The springing and playing of the little animal increased with his increase of strength. I have often observed his sports for a long time and with the greatest delight, and can testify that he never made a false move or a wrong estimate of an exact point which he desired to reach. The little monkey gave indisputable proof that, from the very first, he could judge distances correctly and could determine the degree of force necessary to cover them exactly. He knew his natural movements from the first moment, and knew how to accomplish through them certain ends, where human understanding would have required a long series of trials.

"After about six weeks the monkey began to take other food than the mother's milk and new phenomena appeared. The two animals manifested a wide variation in their mental life. The same mother which formerly took tender care of her young, which without interruption bore him hanging to her body and to her breast, and was so full of mother care, was now ready to snatch the food from out his mouth. The same mother did not permit him, as he began to eat, to lay hand upon but the least morsels of food. As soon as the keeper had given them fruit and bread, she seized it, thrust the young from her, if he approached, and hurriedly filled her cheek pouches, before she left it. One greatly erred if he supposed that a nobler impulse than gluttony had moved her to this action. She could not suppose it necessary for suckling the young, for she had no milk any longer, neither did she entertain any apprehension that the food would be of any harm to it. The young ate the food and flourished on it. Hunger soon made him keen, venturesome and active. He no longer shrank back at the strokes of the mother, and now she pushed him away in order to secure everything for herself. The young was cunning and active enough, indeed, to get a bite and to spring around the back of his mother where he devoured it. This foresight was necessary; for the mother many times went to the farthest corner of the room in order to take the food away from her young. In order to prevent injury, which the unnatural feeling might prompt, we gave them more food than the mother could eat or store in her pouches and thus we helped the young one. It continued in good health and was cherished by the mother so long as it did not take her food."

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